Comments to the SAB on 8-29-18 Draft Quality Review of EPA's Biogenic C Accounting Framework

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Introduction

The Partnership for Policy Integrity and the Center for Biological Diversity appreciate the opportunity to submit comments on the SAB's latest review of EPA's 2014 Framework for Assessing Biogenic CO_2 Emissions from Stationary Sources. We have been involved with this process for about 8 years now, and commented extensively. Progress in making the framework and the assessment by the Biogenic C Panel and the SAB scientifically defensible has been hard-won. This draft contains significant improvements over the last version, particularly with regard to the timeframe issue, which is obviously a question of policy. It is also good to see that the report rejects use of the sprawling economic models (such as FASOM) that previous drafts had relied upon. Unfortunately, however, there are still some vestiges of the former report that confuse its message and contradict the advances that this version has made. The following comments are not comprehensive, but they provide a guide to the aspects of the report that should be changed or eliminated. The report can probably be shortened by another 25% or so, once certain sections are removed.

The SAB critique should acknowledge that the Framework is not fit for purpose, because it is not suited for evaluating net emissions from individual biomass facilities

Earlier reviews of EPA's Framework have continually complained that EPA did not provide enough guidance on how the framework is to be used. But it is right there in the title: "FRAMEWORK FOR ASSESSING BIOGENIC CO2 EMISSIONS FROM STATIONARY SOURCES." As the present document states at p. 6, *"For its review of the 2011 Framework, the SAB requested and was given a policy context for the biogenic CO2 accounting framework. The SAB was told that the 2011 Framework was intended to guide the determination of CO_2 emissions from regulated stationary sources under the Clean Air Act, specifically those facilities receiving a prevention of significant deterioration (PSD) air permit and that were required to conduct a best available control technology (BACT) analysis for CO_2 emissions. The question before the agency, and hence the SAB, was whether and how to consider biogenic greenhouse gas (GHG) emissions in reaching thresholds for permitting and decisions about BACT for CO_2 emissions from bioenergy."*

EPA was clear about the intended purpose of the framework. Yet while the alternative framework that was proposed by the Biogenic C Panel *could* be used to assess emissions at a single facility, this capability has been downplayed in the current document, which has removed all the modeling and scenarios from the previous draft. The present SAB report instead emphasizes the need for big, sweeping approaches to characterizing mass carbon flows at the

"landscape" and "regional" levels (without really saying what this means, in terms of actual scale). Rather than taking this opportunity to put the framework back on track and strengthen its practical utility as a tool EPA can actually use, the advice given by the SAB nearly *rejects* the possibility of using the framework at the facility scale. This is especially ironic given that since 2011 when EPA published its first version of the framework, there have been a number of models published that evaluate net bioenergy emissions at the scale of an individual facility.[1] Unfortunately, the SAB evaluation cites almost none of these papers.

Example of the need for single-facility modeling: Imagine a single new wood-burning power plant is built in New England. It burns 900k tonnes of wood a year, and accordingly emits just over 900k tonnes of CO_2 at the smokestack. It needs an air permit for CO_2 emissions, but (in this imaginary world) EPA wants to write the permit for the "net" emissions, not the stack emissions. Or perhaps this facility is going to be regulated by a carbon-trading program, and policymakers want to charge it for allowances representing net carbon emissions, rather than stack emissions. In both these cases, policymakers would want to know what proportion of each tonne of carbon emitted at the smokestack represents net carbon added to the atmosphere. These are decisions that have to be made up front, when the air permit is written during plant construction, or when the plant is brought under the carbon trading program. To assess net emissions requires some knowledge about the facility's "fuelshed"; estimates of typical standing biomass for fuelshed forests; and typical BAF's for fuels burned (which as the cover letter to the report states, reflect "*a biogenic feedstock's net carbon emissions after taking into account its sequestration of carbon in regrown biomass or soil and emissions that might have occurred with an alternate fate had the biomass not been used for fuel*."). With such information it is possible to develop a weighted estimate of the facility's net carbon emissions.

[1] Booth, M. S. (2018). "Not carbon neutral: Assessing the net emissions impact of residues burned for bioenergy." Environmental Research Letters **13**(3): 035001.

Buchholz, T., J. S. Gunn and D. S. Saah (2017). "Greenhouse gas emissions of local wood pellet heat from northeastern US forests." Energy **141**: 483-491.

Hudiburg, T. W., B. E. Law, C. Wirth and S. Luyssaert (2011). "Regional carbon dioxide implications of forest bioenergy production." Nature Climate Change **1**: 419.

Laganière, J., D. Paré, E. Thiffault and P. Y. Bernier (2017). "Range and uncertainties in estimating delays in greenhouse gas mitigation potential of forest bioenergy sourced from Canadian forests." GCB Bioenergy **9**(2): 358-369.

McKechnie, J., S. Colombo, J. Chen, W. Mabee and H. L. MacLean (2011). "Forest Bioenergy or Forest Carbon? Assessing Trade-Offs in Greenhouse Gas Mitigation with Wood-Based Fuels." Environmental Science & Technology **45**(2): 789-795.

Mitchell, S. R., M. E. Harmon and K. E. B. O'Connell (2012). "Carbon debt and carbon sequestration parity in forest bioenergy production." GCB Bioenergy **4**(6): 818-827.

Walker, T., P. Cardellichio, J. S. Gunn, D. S. Saah and J. M. Hagan (2013). "Carbon Accounting for Woody Biomass from Massachusetts (USA) Managed Forests: A Framework for Determining the Temporal Impacts of Wood Biomass Energy on Atmospheric Greenhouse Gas Levels." Journal of Sustainable Forestry **32**(1-2): 130-158.

In fact, the emphasis on characterizing mass landscape scale carbon fluxes, and the advocacy for the "reference point" approach, come perilously close in our opinion to reinforcing common biomass industry talking points that bioenergy has net zero emissions. These myths are given their ultimate expression in the current EPA policy just enacted under Pruitt, of treating all biomass from "managed forests" as "carbon neutral." This shameful policy is the ultimate culmination of a policy that evaluates carbon flux at the regional level, obscuring the impacts of burning wood for energy under a general supposition that as long as growth exceeds harvest at the regional level, bioenergy carbon impacts are negligible (note, however, the Pruitt memo did not even require forests to be "sustainably" managed). Advocacy for the reference point approach, which was previously unequivocally rejected by the Biogenic C Panel, undermines the credibility of the report. This is unfortunate, because in the present climate of obfuscation of science and alternative facts, a document like this needs to be crystal clear in its scientific integrity, intent and execution.

Further, the document is hard to understand. We have worked on bioenergy carbon modeling for over ten years, and we have spent several hours poring over this document, trying to pull out its method and meaning. We are particularly concerned that members of the SAB who don't work in this field may sign off on the document, assuming their own lack of experience with carbon modeling is responsible for the fact they can't scry its meaning. Trust us: it's not you. Despite significant improvements in this draft, the document is still confusing and self-contradictory. We suggest that anything the SAB doesn't understand in this document should be cut.

Modeling of net emissions is a one-time thing – it can't be retroactively adjusted

In assessing net carbon emissions from individual power plants, EPA needs an approach that will allow it to model and project net emissions when the permit is written – i.e., before the facility is built. This means that all the modeling is done at one time – there is no going back to retroactively "adjust baselines" (as the report calls for), and little if any ability to conduct post-permitting monitoring and verification. EPA is not going to go into the "field" and collect data on how a facility is affecting carbon stocks on the ground; nor does FIA data on forest carbon provide the spatial or temporal resolution necessary to retroactively assess impacts of an individual facility. Carbon modeling to predict net emissions is likely to be one-time thing that is best conducted for an individual facility and the particular fuel mix within its fuelshed.

The document seems confused about landscape vs stand-level analyses

Exacerbating the confusion about assessing carbon stocks on the landscape versus assessing net emissions from a single facility, the document seems unclear about the relationship between "landscape-level" analyses and "stand-level" analyses. Despite the abundance of a number of published models that rely on sequential cutting and regrowth analysis of plots harvested for bioenergy to estimate net carbon emissions – i.e., "stand-level" analyses - the document states in several places that modeling *must* be conducted at the "landscape" level and that a stand-level analysis is not correct. For instance, at page 8 it states, "*Stationary facilities require a*

continuous supply of feedstock, thus a landscape approach for accounting of impacts on carbon stocks is more appropriate than a stand-level approach. "It also states (p. 10) that *"Previous studies have shown that estimates of the effects of biomass harvest on carbon stocks depend on the spatial scale of consideration (stand level or landscape level)."*

This last sentence actually this violates a central premise of modeling – that the model should give consistent results no matter what scale is assessed. If you're getting different answers when you model at different scales, then you're not applying the model consistently.

In fact, as Figure 1 demonstrates below, a landscape level analysis done correctly should produce the same result as a stand-level analysis.

The scenarios in Figure 1 conceive of the landscape as an assemblage of plots. In this simple example, the goal is to generate a BAF for biomass sourced from cutting trees. Trees are cut for fuel in the biomass scenario, and trees continue growing in the counterfactual scenario. We want to know the cumulative difference in carbon stocks between the two scenarios, which informs the BAF. Each plot contains 100 tonnes of carbon. Each year a plot, or a set of plots, is cut for fuel, resetting aboveground stocks to zero. Plots then regrow, adding one tonne of carbon per year (actual regionally specific regrowth estimates can be obtained using forest programs such as the Forest Vegetation Simulator, which uses FIA data). This analysis calculates the difference in carbon stocks between the two scenarios at Year 4. This example demonstrates that it doesn't matter if you compare the whole landscape to its counterfactual, or just those plots that have been cut - the carbon impact, calculated as the difference between the scenarios, is the same.

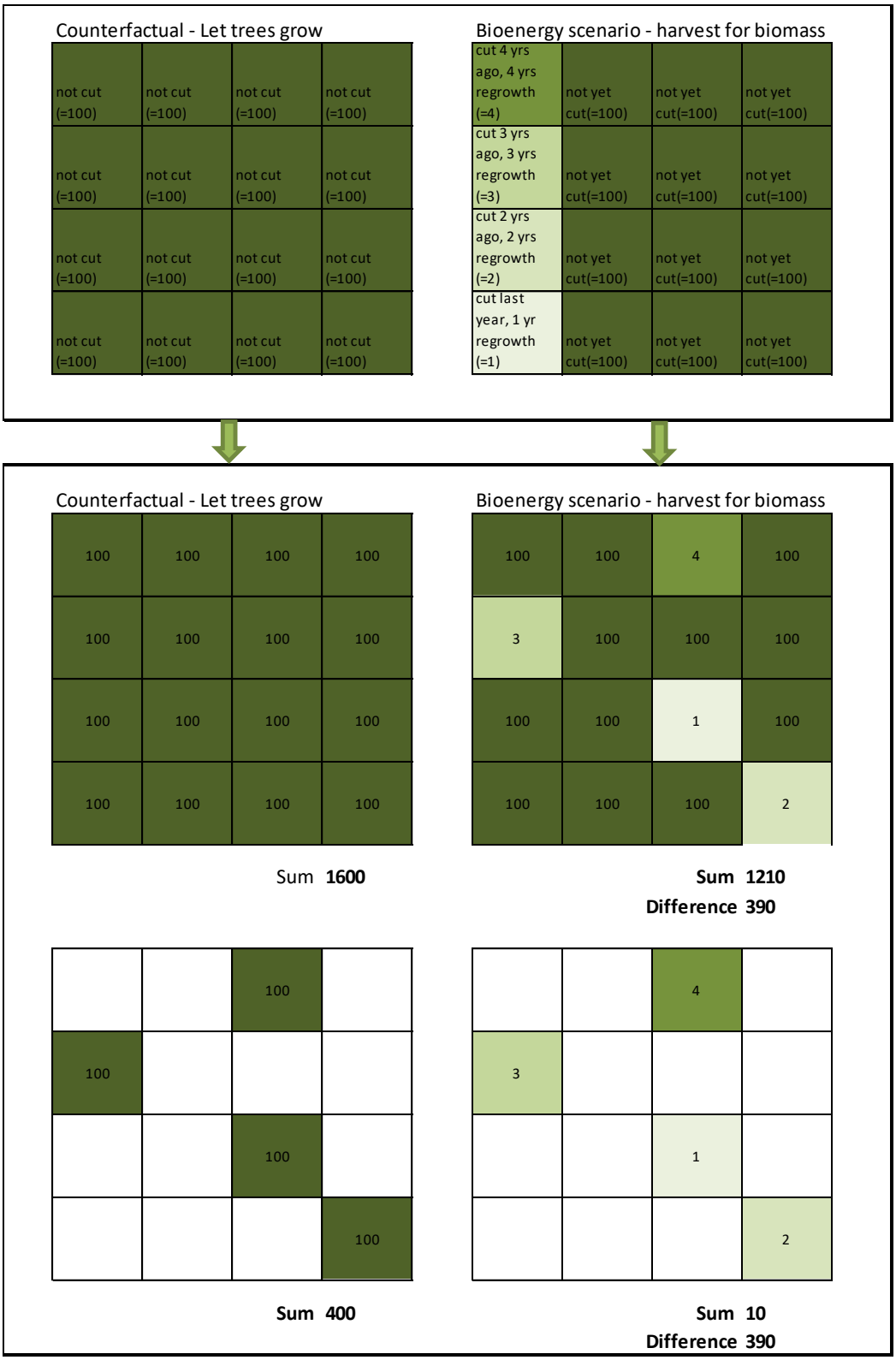

Figure 1a. It doesn't matter if you compute the difference between the bioenergy and counterfactual scenarios for the whole "landscape," or just those plots that are affected by biomass harvesting – the difference in carbon stocks is the same.

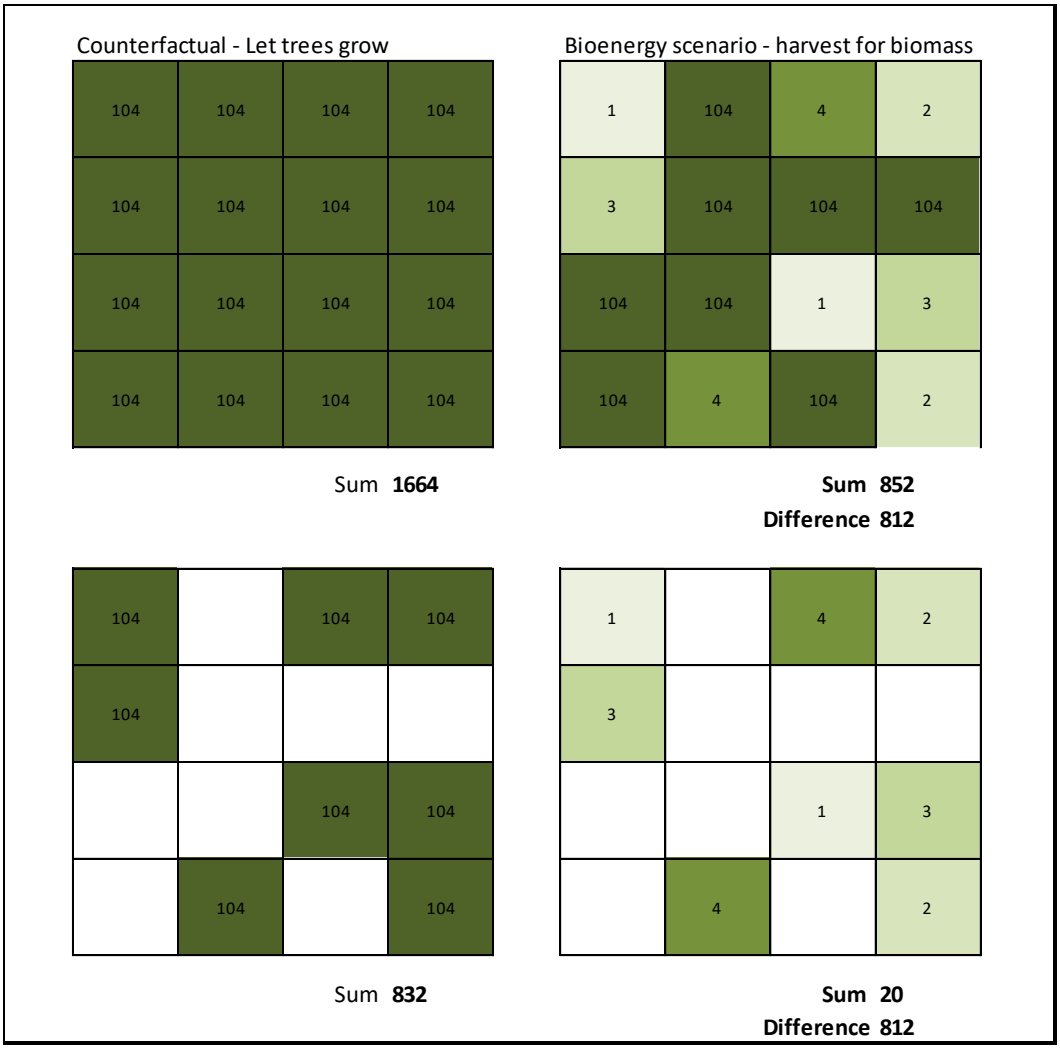

Figure 1b. This version continues "growing" the uncut plots over the four years, incrementing them by one tonne of carbon per year. It also doubles the number of plots that are cut for biomass each year. Again, it doesn't matter if you compute the difference between the bioenergy and counterfactual scenarios for the whole "landscape," or just those plots that are affected by biomass harvesting.

There are several things we can observe from this example.
First, as already stated, since we are interested in the difference in the two scenarios, it is not necessary to model data for every plot on the landscape – only those plots that are affected by biomass harvesting.

Second, while the report states (p. 11) that *"Since stationary facilities require a continuous supply of feedstock, multiple stands will be disturbed in a regulated manner (i.e., completely asynchronously),"* and therefore*, "stand level accounting is not relevant to the calculation of*

BAFs," in fact, sequential analysis that accumulates the effect of harvesting and regrowth of individual plots through time is preferable, because it makes it clear that each plot is "responsible" for re-sequestering equivalent carbon that was released when that plot was cut (there are more problems with the statement from p. 11, as discussed below). However, the model does not need to know *where* on the landscape that forest cutting for biomass happens (i.e., the model does not have to be spatially explicit). While it is helpful to have specific data on plot-level carbon stocks, it is not necessary – an average standing carbon estimate for a facility's fuelshed, plus its regrowth rate, can be used to generate robust modeling results.

Third, you *do* need to know what the types of fuels are (residues versus whole trees, for instance) and the rough amounts burned, because this matters to plant-level net carbon emissions (a weighted average of the BAFs of the fuels burned).

Discussion of "landscape" BAF is incorrect

The cover letter to the document defines the BAF as reflecting "*a biogenic feedstock's net carbon emissions after taking into account its sequestration of carbon in regrown biomass or soil and emissions that might have occurred with an alternate fate had the biomass not been used for fuel*." It is clearly a feedstock-level metric, appropriate for a plot-level analysis.

Yet the framework, and the present document's critique of it, seems to want to develop a BAF for an entire landscape containing all the different types of fuels. At p. 15, the document states "*A shifting projection of the reference baseline that includes a historical time period could be used to reset the baseline periodically based on re-measuring carbon stocks on the landscape, using data from existing inventory programs. This would improve the accuracy of the baseline over time. Future changes in growth-to-harvest ratios could be used to inform the model assumptions and modify the BAF that would be applicable going forward.*"

It is not appropriate to assign a BAF to a landscape. Carbon stocks on the whole landscape are affected by climate, drought, land clearing, harvesting for biomass, harvesting for every other wood product, bugs, and fire. Comparing an anticipated no-bioenergy baseline scenario with bioenergy scenario to a unitary landscape, which contains some or all of the fuels to which BAFs are properly assigned, is impossibly complex and essentially meaningless. The deterioration of the BAF concept from one crisply defining carbon impacts of particular types of biomass, as explained in the cover letter, to one imprecisely and inexplicably applied to entire landscapes, actually makes the document incoherent.

Fuel-specific BAFs do not require information about the amount of fuel harvested

The document is imprecise about the information required to generate a BAF. At p. 16 it states, "*If the EPA's goal is to obtain a region-specific BAF for a feedstock, it will be necessary to project region-specific and feedstock-specific demand for biomass*."

Here, it is clear that the BAF applies to a feedstock, not a landscape, so that is good; it is also legitimate to develop regionally specific BAFs for different fuels, because trees grow at different rates, and residues decompose at different rates, depending on the region. However, it is not necessary to know biomass demand to develop fuel-specific BAFs. You don't need to know how many tonnes of trees will be cut down for biomass to determine the difference in carbon stocks between an uncut stand and one that's been harvested – it's a per-acre metric and what is true for the first stand will be true for all the others.

The discarded "reference point" returns in a new incarnation – but should be abandoned for good

The reference point approach, which was largely rejected by the SAB in previous incarnations of its review, has reappeared in this version of the document. The 2-18-2016 version of the SAB's draft report expresses strong reservations about the reference point approach, p. 10: "*To compare change in any system over time, there must be a baseline against which to assess changes so that two distinct scenarios can be compared. The EPA's reference point baseline approach simply assesses the estimated net change in land-based biogenic CO2 fluxes and/or carbon stocks between two points in time. In our 2012 SAB report, we stated that the* ***reference point baseline approach is inadequate*** *in cases where feedstocks accumulate over long time periods because it* ***does not estimate the additional effect of a stationary facility's combustion of biomass on carbon emissions over time****. The EPA has acknowledged this limitation in its 2014 Framework and now includes a future anticipated baseline analysis alternative along with a reference point approach.* ***The SAB remains concerned that the reference point approach has important limitations and should not be the preferred approach***."

Page A-4 of that 2016 SAB report explains the objections of the Biogenic C Panel to the reference point approach: "*The SAB Panel expressed concern that the reference point baseline* ***does not address the important question of additionality****, or what would have been the trajectory of biogenic CO2 stocks and fluxes in the absence of an activity or activities using biogenic feedstocks for energy, especially in the context of forest-derived feedstocks. '****Estimating additionality****, i.e., the extent to which forest stocks would have been growing or declining over time in the absence of harvest for bioenergy,* ***is essential****, as it is the crux of the question at hand.* ***To do so requires an anticipated baseline approach'****" (SAB Letter, p. 2)."*

However, despite these warnings as to the total inadequacy of the reference point approach, it has persisted and even evolved in the present document, which newly states at p. 2, "*The reference point approach, if adjusted at regular intervals (e.g., every 5 to 10 years) to account for any additional regional sequestration, would address the SAB's earlier concerns, allowing for the more direct establishment of a baseline while capturing additional increases in carbon stocks*." At p. 16, the document formally recommends adopting the discarded approach: "*The SAB suggests consideration of two cumulative BAFs—that proposed by EPA and an alternative metric that takes into account the changes in terrestrial carbon stocks over time. The appropriate cumulative metric for calculating BAFs will depend on the understanding of the carbon system and climate response for which there is uncertainty*."

(we are unclear what that last sentence means. What is the metric that would determine which approach to use?)

This is a big problem. First, the reference point approach, which compares carbon stocks between two points in time on a landscape, is incapable of truly assessing the carbon impacts of bioenergy, as the SAB and Panel have noted. It is also incompatible with the feedstock-level BAF approach that is endorsed elsewhere in the report, which can be used to determine net emissions from biomass facilities.

Second, when comparing carbon stocks on the landscape between two points in time under the reference point approach, what is to be done about all the other things that affect carbon stocks – harvesting, planting, landuse change, fire, drought, pests… how is the bioenergy "signal" to be determined?

Third, "adjustment" (of what we're not told) to "account for additional regional sequestration" will not address the problems with the reference point approach. Nothing about this solution addresses the fact that the reference point approach does not take into account the trajectory of forest carbon stocks in the absence of biomass harvesting. As the Biogenic C panel and the SAB have pointed out, "*Estimating additionality, i.e., the extent to which forest stocks would have been growing or declining over time in the absence of harvest for bioenergy,* ***is essential, as it is the crux of the question at hand****. To do so* ***requires*** *an anticipated baseline approach."*

The Biogenic C Panel, which has literally worked on this issue for years, was very clear in their rejection of the reference approach. To ensure the scientific credibility of the report, the SAB should reject the approach as well.

The document contains alarming echoes of biomass industry talking points

In some places, the report is clear that fuel harvesting generally involves net emissions that are gradually offset as plots regrow. Yet the report still retains biomass industry talking points that claim carbon neutrality for biomass as long as net growth is positive. For instance, at p. 11, the document implies that a landscape analysis is necessary because too much is happening at the same time (p. 11): *"Since stationary facilities require a continuous supply of feedstock, multiple stands will be disturbed in a regulated manner (i.e., completely asynchronously), and the order in which losses and gains occur becomes meaningless at the landscape level because both simultaneously occur; thus, the operative issue is the overall balance between losses and gains of carbon at the landscape scale. Thus, stand level accounting is not relevant to the calculation of BAFs. If harvest does not exceed the rate of carbon accumulation, the landscape-level carbon stocks are stable or increasing."* (similar language at p. 9).

First, an important note: while the document states "stand-level accounting is not relevant to the calculation of BAFs," that's true – but it's true because a BAF is scale-free. E.g., to use the example from above, the difference in carbon stocks on a per-acre basis between a harvested,

now re-growing forest, and a forest that's never been cut, is going to be the same for one acre, or 100 acres.

Second, this language is misleading because it implies that sequestration occurring on plots cut in previous years is credited with offsetting the carbon released by current fuel-burning (which would be double-counting). It is particularly alarming to see the phrases "*the operative issue is the overall balance between losses and gains of carbon at the landscape scale," and, "if harvest does not exceed the rate of carbon accumulation, the landscape-level carbon stocks are stable or increasing,"* because this language is so similar to language from an article co-authored by Jennifer Jenkins, who authored the first version of the EPA framework but now works for Enviva, the largest wood pellet manufacturing company in the world.[2] In her job at Enviva, Jenkins routinely claims things like "*Energy from biomass harvested from managed working forest landscapes where growth outpaces harvest is zero-net-carbon energy*," because *"For a working managed forest landscape, at any given time, across all the different stands that make up that landscape, the forest is yielding emissions from those units being harvested while simultaneously sequestering carbon as a result of new growth and regeneration in other units harvested previously."*[3] Sound familiar? This is similar to the language found in the SAB report. It is absolutely in the interest of the wood pellet industry to be able to claim that as long as forests show net positive growth, bioenergy has no net emissions. This is a seductive concept, but one that the biogenic C panel and the SAB have already rejected. We strongly suggest that this language be removed from the SAB report.

Such an approach is incompatible with science-based carbon accounting that counts emissions and sequestration separately, and is incompatible with the SAB's own definition of the BAF as reflecting "*a biogenic feedstock's net carbon emissions after taking into account its sequestration of carbon in regrown biomass or soil and emissions that might have occurred with an alternate fate had the biomass not been used for fuel*" – a definition that clearly requires each plot to be "responsible" for re-sequestering equivalent carbon as was released by its earlier harvest. This objectionable section on p. 11 (and p. 9) should simply be deleted to avoid flirting with the fallacy that bioenergy emissions can be "instantaneously carbon neutral" due to carbon uptake occurring "somewhere", a favorite bioenergy industry claim.

It is also alarming to see the document refer to "growth to harvest ratios" as a modifier for BAFs (p. 15), as if this metric is somehow relevant to the carbon impacts of bioenergy. This is a biomass industry talking point (bioenergy should be treated as having net zero emissions as long as growth exceeds harvest). It does not belong in a scientific report.

[2] In retrospect, EPA asking Jen Jenkins to craft the biogenic C framework was like having Michael Flynn design your election security plan.

[3] https://www.energycentral.com/c/ec/climate-solution-we-cannot-afford-ignore-biomass-sourced-naturally-managed

Document needs to be edited for confusing economic modeling jargon

Despite rejection of large complex economic models (which is good) the document retains vestiges of Big Modeling jargon from previous incarnations. For example, at p. 1 it states, "*In general, the BAF for a class of feedstock should be estimated for the average effect of the last increment of demand for that feedstock*."

It is not clear: Why is this statement here - just generally, why should this be true? There are a number of published models that calculate net carbon impacts of biomass without resorting to economic gimmickry around "increments" of feedstock. Further, this statement does not really make sense. How can *one* increment of feedstock have an "average effect"? Wouldn't the "average" effect refer to the average of *many* increments of feedstock, leading up to that point? And what is the "last increment," anyway, given that biomass plants demand fuel on an ongoing basis in order to operate?

The document should be stripped of all such jargon and all necessary remaining concepts expressed in a simple way so that special economic or modeling expertise is not required to understand the meaning.

More confusion arising from economic modeling

Another vestige of the economic modeling approach is the incorporation of unjustified assumptions about how facilities operate (consistent with the "rational operators" that economic models assume). For instance the following at p. 17: *"In the absence of a mandate for use of specific feedstocks or incentives for specific types of bioenergy which might be prescribed in a policy framework, and which would inform the feedstock-specific demand that should be modeled, a reasonable approach is to model the aggregate demand for feedstocks.* ***This approach assumes facilities are constantly seeking their least-cost alternative.*** *An aggregate demand could be imposed on the model and used to determine demand for different feedstocks in different regions. This would allocate demand across feedstocks as well as within each category to simulate a given target aggregate demand determined by the market's ability to draw from the least cost combination of feedstocks."*

This is not realistic. While of course bioenergy operators seek the lowest-cost fuels, they don't burn just whatever fuel becomes available. Wood burning power plants cannot switch to burning corn stover, for instance, without significant adjustments to their operation and emissions controls. All such assumptions – and remaining references to large economic modeling efforts – should be carefully vetted or removed.

No acknowledgement of methane from fuel piles

Finally, one small but important point. Methane emissions have been shown to be significant from wood chip piles at biomass plants, as well as wood pellet storage and transport. The report talks about the importance of including methane emissions in counterfactual scenarios (e.g. "methane emissions for woody mill residuals," p. 2), which would presumably make burning

materials for biomass look more climate-friendly by comparison, but does not mention that collecting wood from the forest and storing it in enormous piles at biomass plants may itself be a source of emissions (some citations at http://iopscience.iop.org/article/10.1088/1748-9326/aaac88).

Thank you for the opportunity to comment.

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